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ABSTRACT

The primary objective of the Title I Reading Project was to improve the reading skills of sixth- and seventh-grade underachievers. Treatment consisted of individualized programs based upon diagnostic testing in reading. Project classes in nine and 11 schools at the sixth- and seventh-grade levels, respectively were evaluated. Selected pupils were randomly assigned to experimental and control groups within each school. Analysis of the pretest scores revealed that attrition had operated to somewhat bias the samples by the end of the study. These biases tended to favor the experimental groups. The results of the study indicated that project children did no better or worse on the criterion reading skills tests than did control students. Attitudes of project and control pupils were also similar. It was felt that there was a possibility that the difficulty level of the criterion test was inappropriate for these groups. To test this possibility, and the possibility that the program had delayed benefits, a followup study of the same groups was planned to be completed during the 1972-73 school year. Tables and appendixes are included. (Author/AW)

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TITLE I
READING PROJECT
1970-71

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RESEARCH DEPARTMENT
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ABSTRACT

The primary objective of the Title I Reading Project was to improve the reading skills of sixth- and seventh-grade pupils who fell within "normal" IQ ranges but who were identified as reading below their potential.

Treatment consisted of individualized programs based upon diagnostic testing in reading. Each student attended a project class one period a day. Project teachers all met at least minimum requirements in reading instruction.

Project classes in nine and eleven schools at the sixth- and seventh-grade levels respectively were evaluated. The Research Department applied semi-empirical criteria in selecting a parent population. These pupils were reading below their potential on the basis of available IQ and reading test scores. Pupils in this population were randomly assigned to experimental and control groups within each school according to "quota" requirements.

Analysis of pretest scores revealed that attrition had operated to somewhat bias the samples by the end of the study. These biases, however, tended to favor the experimental groups.

The results of the study indicated that project children did no better or worse on the criterion reading skills' tests than did control students. Attitudes of project and control pupils were also similar.

There is a possibility that the level of difficulty of the criterion test was inappropriate for these groups. To test this possibility, and the possibility that the program had "delayed" benefits, a follow-up study of the same groups will be done in the early Fall of 1971. The follow-up study will be completed in time to assist administrative decision-making regarding uses of Title I funds during the 1972-73 school year.

TITLE I READING

The Title I Reading Project was designed to deal with sixth- and seventh-year students who are reading below their potential and who may, therefore, have difficulties adjusting to the departmentalization of the junior high setting.

Selection of Participants

Defining who should participate in the project was an important aspect of sample selection. Not all students read at grade level. A high IQ student, for instance, might be expected to read at an eighth- or even ninth-year level. The reverse may be true for lower IQ students. To take a sixth-year reading level as the "potential" for all sixth-year students is patently false.

A better estimate of this potential is the mean reading level for sixth graders at each IQ level. If a student in the sixth grade is reading at a lower level than the average of all other sixth-year students who have the same IQ, then he may be said to be reading "below his potential."

It is this definition that was used in selecting the sample for the Title I Reading Project.

Articulation cards were gathered on all students entering the sixth and seventh grades in Broward County. These cards provided scores obtained at the end of the fifth grade on the Otis Short Form Intelligence Test and the California Test of Basic Skills. For each grade regression procedures were used to estimate a reading grade equivalent score for each IQ level from 80 to 108. (These limits were decided upon by the project directors. It was felt, for instance, that while a student with a 75 IQ may be reading below his potential, a substantially different approach would be needed to help correct his difficulty. This project was designed to help the underachiever with a relatively "normal" potential.)

Table 1 presents the estimates of mean reading levels for each IQ for the sixth and seventh graders in Broward County junior high schools.

What the table indicates is that a student going into the sixth grade with an IQ of 87 would be predicted to read on the 3.6 level if he were average for his IQ group.

The realities of operating in an ongoing school system added a number of further constraints to the selection process. These constraints made it necessary to select the range of reading deficiency used in such a way that there would be an appropriate number of eligible students at each school to fill the classes of the previously assigned project teacher(s). At the same time, an adequate number of students, also meeting the criteria, had to be left for assignment to the control group.

Table 1

Mean Reading Level for Each IQ

IQ	6th Grade	7th Grade
80	2.7	3.5
81	2.8	3.6
82	2.9	3.8
83	3.1	3.9
84	3.2	4.0
85	3.5	4.2
86	3.5	4.3
87	3.6	4.5
88	3.7	4.6
89	3.9	4.8
90	4.0	4.9
91	4.1	5.0
92	4.3	5.2
93	4.4	5.3
94	4.5	5.5
95	4.7	5.6
96	4.8	5.8
97	4.9	5.9
98	5.1	6.0
99	5.2	6.2
100	5.3	6.3
101	5.5	6.5
102	5.6	6.6
103	5.7	6.8
104	5.9	6.9
105	6.0	7.1
106	6.1	7.2
107	6.3	7.3
108	6.4	7.5

The following range of reading levels was found to fulfill the needs of the project centers: the sample consisted of students who were between 0.5 years and 2.5 years behind the mean reading grade equivalent for their IQ (within the 80 to 108 IQ range).

Table 2 gives the Reading Grade Level range for each IQ for the sixth and seventh grades. For example, the table indicates that students going into the seventh grade with an IQ of 95 must have a reading level of between 3.1 and 5.1 years in order to be eligible for the program.

From the list of students meeting this criteria in each school, enough students were randomly chosen to fill the required number of reading classes to a maximum of 80% of capacity. Approximately the same number of students were randomly assigned to control groups.

Table 2

Reading Grade Level Range for Each IQ

IQ	6th Grade Lower Limit	6th Grade Upper Limit	7th Grade Lower Limit	7th Grade Upper Limit
80	1.0	2.2	1.0	3.0
81	1.0	2.3	1.1	3.1
82	1.0	2.4	1.3	3.3
83	1.0	2.6	1.4	3.4
84	1.0	2.7	1.5	3.5
85	1.0	3.0	1.7	3.7
86	1.0	3.0	1.8	3.8
87	1.1	3.1	2.0	4.0
88	1.2	3.2	2.1	4.1
89	1.4	3.4	2.3	4.3
90	1.5	3.5	2.4	4.4
91	1.6	3.6	2.5	4.5
92	1.8	3.8	2.7	4.7
93	1.9	3.9	2.8	4.8
94	2.0	4.0	3.0	5.0
95	2.2	4.2	3.1	5.1
96	2.3	4.3	3.3	5.3
97	2.4	4.4	3.4	5.4
98	2.6	4.6	3.5	5.5
99	2.7	4.7	3.7	5.7
100	2.8	4.8	3.8	5.8
101	3.0	5.0	4.0	6.0
102	3.1	5.1	4.1	6.1
103	3.2	5.2	4.3	6.3
104	3.4	5.4	4.4	6.4
105	3.5	5.5	4.6	6.6
106	3.6	5.6	4.7	6.7
107	3.7	5.7	4.8	6.8
108	3.9	5.9	5.0	7.0

Placement of the remaining 20% was left up to the principal's discretion. In about one-third of the schools the principal was provided with a list of students who met the criteria but were simply not chosen in the random sample for either the experimental or control group. If no list was provided, the classes could be filled with students new to the county as long as they were tested and found to meet the criteria. Students assigned to project classes by the principal were not used in the evaluation. In no case were control students to be assigned to the experimental condition.

The actual assignment of students to particular project classes was left up to the junior high principals themselves in accordance with the needs of their schedules.

Sampling Procedures

Assignment to Treatment Groups

The most important aspect of the sampling procedures from a research standpoint was the random assignment of pupils to treatment groups within schools. The number of experimental classes to be filled and the number of eligible students at each school imposed some practical restraints upon assignment of equal numbers to both treatment conditions. No attempt was made to stratify students by pretest scores in the randomization procedures. Computer-generated random numbers were utilized in assigning pupils. Multiple passes were made until quotas were filled. All students had the same chance to be assigned to either group on each pass.

It should be noted here that the initial criteria for selecting the parent pool for random assignment had very definite limitations. There are many reasons why such procedures would lead to misclassifications. Some of the misclassified pupils were, in some cases, removed from the experimental program by school personnel. Such transfers would not occur in control groups, so pupil transfers could conceivably have biased the original sample. For this reason, transfers were discouraged except in conspicuously necessary cases.

Procedures were instituted to permit checking the sources of sample attrition.

Attrition

Attrition was defined in terms of being present at the posttesting session as a member of the group to which the pupil was originally assigned. Slightly more than one-third of the original parent pool was lost due to attrition. Pupils were assigned to the parent pool prior to the opening of school. Pupils who moved during the summer or attended other schools could, therefore, not be included in the study. Absenteeism from the posttesting session, however, was found to be the greatest single source of attrition. Analyses of variance procedures were applied to pretest scores to estimate the effects of the attrition.

The effect of attrition upon pretest scores at the seventh-grade level seemed to be dependent upon the particular school and treatment group (experimental or control). A significant three-way interaction was found among these three factors. At the sixth-grade level a two-way interaction was found between treatment groups and attrition. Attrition had little effect upon scores obtained by the control group. Students still in the experimental groups at the time of posttesting scored higher than the control group and the pupils who were no longer in the experimental group. The difference in pretest scores was greatest between the two experimental groups (there versus not there).

Close inspection of the seventh-grade data revealed that biasing factors were generally working to favor the experimental group. The investigation of the effects of attrition upon the original sample, therefore, indicated biases which favored the experimental groups, especially at the sixth-grade level. There were no significant pretest differences between treatment groups prior to attrition.

Testing

As previously mentioned, the pretest data were constituted by scores on:

1. Otis Short Form Intelligence Scale.

2. Reading score on the California Test of Basic Skills, Level II.

These tests were administered during the countywide testing in the spring of 1970.

Posttesting was conducted during the first week in May 1971. It consisted of:

1. Reading Vocabulary and Reading Comprehension sections of the California Test of Basic Skills, Level III.

2. A 16-item attitude and opinion questionnaire created using suggestions from project teachers.

Posttesting was carried out on a schoolwide basis. That is, all students in both the control and the experimental groups in any school were tested simultaneously in one session. No provision was made for make-up exams for absentee students.

Treatment

The most important part of any research project of this type is an adequate description of the treatment. The reader must be told *what actually happened to pupils in the treatment groups*. Unfortunately, in the present case this is almost impossible. While the type of student and the equipment available were pretty much the same for all schools, the actual program that was implemented depended on a number of additional factors. These were such things as:

1. The physical size and arrangement of the project classes. In some cases a regular classroom was used, while in other schools only a small back room in the library was available.

2. The subject from which students were taken in order to attend project classes was not the same in each school. In some cases students were pulled from English, while in other instances students missed gym in order to be in Title I Reading. This may have had the paradoxical effect of lowering some students' motivation (because they are missing gym), while in effect giving them a double dose of language arts.

Perhaps the most important factor to be considered in the context of this study is the manner in which the control group was treated. In many cases, instead of simply allowing control students to participate in a "normal" school program, these pupils were also put into special reading programs. While this was undoubtedly done with the best of intentions (after all, these students were identified as reading below their

potential), the actual effect may have been to blur the distinction between participation and nonparticipation in the Title I Reading Project. However, it can be argued that if some control pupils received similar services and results did not differ, Title I funds could be better allocated to providing more unique benefits.

Project classes held a minimum of 12 and a maximum of 15 students. Classes met once each day for one class period averaging 50 minutes.

Teachers hired for this project were required to have special experience in teaching reading and had to have a thorough understanding of diagnosis and instruction in an individualized setting. Each student was tested by the project teacher, using the Stanford Diagnostic Reading Test, an instrument specifically designed to point out reading difficulties. Using the results of this test, a highly individualized program was worked out for each student using a multimedia approach.

The material available included Basal Readers, SRA Reading Kits, multilevel paperback books, programmed workbooks, and mechanical and electronic devices such as the Flashex and Hoffman Reader. After assessing a student's area(s) of weakness, the teacher tried to program him into material appropriate not only in difficulty, but also of a motivational and interest level in keeping with his learning style. Following the guidelines set down by the county, an attempt was made to keep the orientation "phonic" in nature.

In contrast to some projects of this type, the directors of Title I in Broward County did not create a long list of unrealizable and untestable goals. The program was designed simply to improve the reading ability of participants.

Results

CTBS Reading Test Scores

The basic hypothesis of the study was that pupils who participated in the Title I program would score higher on the reading skills measured by the standardized test than the control group. Analyses of covariance procedures were used to test this hypothesis.*

The basic research question to be answered by the statistical procedures was: did participation in the project enable students to score higher on the posttests than could be expected on the basis of their pretest scores? Stated as a problem in prediction, the hypothesis would be: does knowing both a person's pretest scores and whether he was in the experimental or control group make for significantly greater predictive accuracy in estimating his posttest scores than could be obtained from merely knowing his pretest scores but not his group membership?

* A version of a MANOVA program refined by Eliot Cramer, and a locally modified version of an SSP regression analysis program were used in making data analyses.

The hypotheses were made a little more complicated by the fact that knowledge of the school a pupil attended might also be important. The analyses which were run took into account school differences, treatment classifications, and pretest results.

It was found that posttest scores depended upon differences in pretest scores which "interacted" with school differences. In some schools pupils who made high pretest scores did better than pupils who made similarly high scores in other schools. Low-scoring pupils in these same schools did just the opposite. In brief, some of the differences between schools depended upon how well students did on the pretests. In some schools persons with high scores on the pretest did better, but those with low scores worse than their counterparts in other schools.

It was found that the treatment classification seemed to be independent of these interactions between schools and scores on the pretests. Within each school scores of students in both the experimental and control groups differed by similar amounts at all levels of pretest scores. Both groups followed the trend common to their particular school.

These interactions did, however, make it necessary to consider differences between treatment and control groups separately at each school. Had these interactions not been found, scores obtained by all experimental and control pupils could have been compared in one overall analysis.*

Comparisons broken down by individual schools are presented in Tables 3 and 4. The numbers presented in these tables represent differences in raw scores (the number of correctly answered items on each test). The scores are "adjusted" in the sense that pupils in the experimental group would be expected to differ from pupils in the control group by this amount at all levels of the pretest scores. In other words, any two students with the same IQ and pretest reading scores would be predicted to differ by these amounts if one were in the experimental and the other in the control group. This difference would be unaffected by whether their pretest scores were high or low.

Inspection of the tables indicates only three instances of statistically significant differences. Two of the statistically significant differences were in the same school at the sixth-grade level and were in favor of the control group. In School 4, also on the sixth-grade level, the experimental group scored statistically significantly higher than the control group. None of the differences at the seventh-grade level reached the conventional .05 level of statistical significance.

Forty comparisons were made, each with a probability of occurring by chance five percent of the time. At these odds, little importance can be attached to the fact that three significant results were obtained.

* The details of the statistical procedures are briefly described in the Appendix for the benefit of technically oriented readers.

Table 3

Differences Between Experimental and Control Group Means
on Posttest Scores Adjusted for IQ and
Pretest Reading Scores (6th Grade)

	School 1		School 2		School 3		School 4		School 5	
	Voc.	Comp.	Voc.	Comp.	Voc.	Comp.	Voc.	Comp.	Voc.	Comp.
Difference in Mean Scores	2.37	2.91	0.59	0.26	1.00	0.56	3.52	1.35	1.52	1.30
In favor of	Control	Control	Exp.	Control	Control	Exp.	Exp.	Exp.	Exp.	Control
	P=.051	P=.032					P=.001			

	School 6		School 7		School 8		School 9	
	Voc.	Comp.	Voc.	Comp.	Voc.	Comp.	Voc.	Comp.
Difference in Mean Scores	0.51	0.04	1.40	0.50	0.48	0.93	1.67	0.04
In favor of	Control	Exp.	Exp.	Exp.	Control	Exp.	Con. trol	Con. trol

Table 4

Differences Between Experimental and Control Group Means
on Posttest Scores Adjusted for IQ and
Pretest Reading Scores (7th Grade)

	School 1		School 2		School 3		School 4		School 5		School 6	
	Voc.	Comp.	Voc.	Comp.	Voc.	Comp.	Voc.	Comp.	Voc.	Comp.	Voc.	Comp.
Difference in Mean Scores	2.28	0.71	2.34	2.44	0.57	1.13	1.35	1.23	0.61	0.48	1.45	1.49
In favor of	Exp.	Control	Control	Control	Exp.	Exp.	Exp.	Exp.	Exp.	Control	Control	Exp.

	School 7		School 8		School 9		School 10		School 11	
	Voc.	Comp.	Voc.	Comp.	Voc.	Comp.	Voc.	Comp.	Voc.	Comp.
Difference in Mean Scores	0.19	1.05	0.73	0.08	0.01	1.12	1.18	0.03	0.71	0.23
In favor of	Exp.	Control	Exp.	Control	Exp.	Control	Control	Control	Control	Control

In short, the Title I Reading Project seemed to have no effect upon the achievement of students as measured by the Reading section of the CTBS. Students in experimental classes did no better or worse than their counterparts in the control group.

Attitude Questionnaire

As was stated previously, the program was designed simply to improve the reading ability of underachieving sixth- and seventh-year students. There were no formal attitudinal goals. This does not mean, however, that some attitudinal change, sought or unsought, would not occur as a result of participation in the project. It is believed by many that underachievement in the reading area is a primary source of unhappy attitudes. For this reason, an attempt was made to assess students' attitudes at the end of the school year.

Both experimental and control students answered a 16-item questionnaire. Table 5 gives the individual questions and percentage of pupils in both groups answering in each of the four response categories. Percents do not sum to 100 because a fifth "no response" category was not reported in these tables. Because of the nature of the data, the table is not broken down by schools, but simply into sixth and seventh grades. A chi-square test was performed on each question to see if there was a significant difference between control students and experimental students. Of the 32 comparisons, only one proved to be statistically significant. This "lonely soldier" was Item 12 for the seventh grade. Since at least one item could be expected to be significant by chance alone, the probability is that the practical significance of this finding is nil.

Apparently participation in the program did not affect the responses of students to the attitudinal questions.

Table 5

Percentage of Sixth- and Seventh-Grade Control and Experimental Students in All Schools Answering in Each of Four Response Categories

A. Very strong agreement
B. Some agreement
C. Slight agreement
D. Disagreement

1. This year school has been very enjoyable.

	6th Grade				7th Grade			
	A	B	C	D	A	B	C	D
Control	43.5	21.5	19.8	13.4	30.7	27.1	20.4	20.0
Experimental	47.1	20.3	20.3	10.3	34.4	27.0	18.8	17.6

2. The teachers this year have been very good.

6th Grade

	A	B	C	D
Control	33.8	22.0	27.4	15.0
Experimental	36.0	28.3	20.6	12.2

7th Grade

	A	B	C	D
	28.3	22.4	29.1	18.5
	23.3	26.3	27.7	20.8

3. Homework is easier now than it was in the past.

6th Grade

	A	B	C	D
Control	24.1	15.0	22.5	36.5
Experimental	24.9	14.1	16.0	42.5

7th Grade

	A	B	C	D
	20.4	17.3	20.4	40.5
	18.3	13.1	20.8	45.9

4. Reading books is a lot of fun.

6th Grade

	A	B	C	D
Control	45.1	13.9	24.1	15.0
Experimental	54.0	14.5	16.0	12.6

7th Grade

	A	B	C	D
	31.8	17.3	20.8	28.7
	39.4	18.3	21.0	19.3

5. Going as far as you can in school is important.

6th Grade

	A	B	C	D
Control	79.0	8.0	7.5	3.7
Experimental	82.7	7.6	3.4	3.8

7th Grade

	A	B	C	D
	84.2	7.0	3.9	3.5
	87.8	4.4	2.4	3.4

6. This year my grades are better than they have been in the past.

6th Grade

	A	B	C	D
Control	37.0	19.8	17.7	23.6
Experimental	41.3	18.3	18.0	19.9

7th Grade

	A	B	C	D
	26.7	18.8	18.5	34.6
	30.7	22.0	17.1	28.2

7. If you are not doing well in one subject in school, it is a good idea to get special help.

6th Grade

	A	B	C	D
Control	63.4	17.2	11.2	5.9
Experimental	70.8	13.0	8.4	4.9

7th Grade

	A	B	C	D
Control	61.8	16.1	12.9	7.4
Experimental	67.2	17.1	8.1	5.7

8. The teachers in this school treat me fairly.

6th Grade

	A	B	C	D
Control	35.4	24.7	14.5	22.5
Experimental	36.0	21.4	20.6	18.3

7th Grade

	A	B	C	D
Control	27.9	27.9	16.9	25.9
Experimental	28.0	22.8	21.5	25.3

9. Most of the time I would rather read a good book than watch television.

6th Grade

	A	B	C	D
Control	19.8	19.3	18.2	40.8
Experimental	21.0	16.0	17.6	42.9

7th Grade

	A	B	C	D
Control	18.5	11.8	20.0	48.4
Experimental	18.1	13.3	18.8	47.8

10. My teachers help me as much as they can.

6th Grade

	A	B	C	D
Control	55.3	13.9	18.2	10.7
Experimental	55.5	18.3	14.5	9.1

7th Grade

	A	B	C	D
Control	39.7	23.2	20.8	14.9
Experimental	41.6	22.5	20.5	13.1

11. I find it easier to read my school books this year.

6th Grade

	A	B	C	D
Control	39.7	22.5	20.9	14.5
Experimental	48.2	21.4	18.7	9.1

7th Grade

	A	B	C	D
Control	32.2	25.9	25.5	14.9
Experimental	31.2	27.2	23.8	15.8

12. I am good friends with most of the kids in my classes.

6th Grade

	A	B	C	D
Control	56.9	17.2	13.9	9.1
Experimental	63.2	18.0	9.9	6.5

7th Grade

	A	B	C	D
Control	53.9	22.0	11.8	9.8
Experimental	62.0	24.0	6.9	5.2

Significant of $\alpha = .05$

13. I like Language-Arts (Reading and English) very much.

6th Grade

	A	B	C	D
Control	42.4	18.2	19.3	18.2
Experimental	45.2	19.9	18.7	13.7

7th Grade

	A	B	C	D
Control	29.1	23.2	20.0	26.3
Experimental	32.5	27.0	18.3	20.3

14. I like Science very much.

6th Grade

	A	B	C	D
Control	43.5	16.1	15.5	23.1
Experimental	42.5	14.1	14.9	26.0

7th Grade

	A	B	C	D
Control	38.5	20.0	18.5	21.6
Experimental	39.4	16.6	18.3	23.8

15. I like Mathematics very much.

6th Grade

	A	B	C	D
Control	45.1	15.0	17.2	20.9
Experimental	47.8	14.5	14.5	20.6

7th Grade

	A	B	C	D
Control	39.7	20.4	16.1	22.4
Experimental	33.7	19.3	18.3	26.7

16. I like Social Studies very much.

6th Grade

	A	B	C	D
Control	39.7	20.4	17.2	20.9
Experimental	42.5	13.0	15.7	26.0

7th Grade

	A	B	C	D
Control	32.2	18.5	18.5	29.5
Experimental	24.3	18.3	20.0	35.4

Discussion and Limitations

Limitations

True experimentation is almost impossible to conduct in a school system. The pressures and requirements of running an ongoing educational program make it difficult to meet the stringent demands of the researcher. As a result, most educational research is of the "field" variety. That is, one attempts to evaluate the program as it is, not as it would have been set up under ideal circumstances. Because of the lack of complete control, and because the treatment is not consistent from time to time, and place to place, results are often difficult to interpret.

This study had three serious limitations:

1. The programs were not monitored by research personnel to secure objective indices of whether the treatments offered to the two groups really differed in any substantial sense.

2. The battery of tests was not complete enough. Further, the criterion measure was possibly too difficult a test for these groups.

3. Provisions should have been made for testing students who missed the posttest sessions due to absenteeism or for other reasons.

Discussion

The most salient of the above limitations from the standpoint of this investigation is the one regarding the criterion test. The complete absence of a program effect leads one to suspect that some sort of artifact might be present in these data. Using too difficult a test could produce results like these, when a more sensitive test would reveal that considerable differences did exist between the treatment groups. This lurking suspicion constitutes a major reason for the evaluative procedures which will be followed during the 1971-72 school year.

At present, these findings appear to be quite unambiguous. The evidence collected in this document indicates that participation in the Title I Project had little effect upon the reading skills measured by the criterion test. Student attitudes also appeared to be unaffected by participation in the project.

Unfortunately these findings were not available at the time the 1971-72 Title I programs were being developed. It would seem in light of these findings and the previous evaluations of this project's predecessor,* that major changes should be made in planning Title I programs for the 1972-73 school year.

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* Title I funds at this grade level had been previously used to support Project GAIN (Gearing Academics to Individual Needs). See report Nos. 1, 19, 32, and 34 for evaluations of this project.

The Research Department has a contractual obligation to provide an evaluation of this project for the 1971-72 school year. This obligation will be fulfilled by retesting the sample groups whose results are reported in this document. Retesting will be done early in the Fall of the 1971-72 school year. This retesting will serve the following functions:

1. It will permit assessing the effects of this program in terms of factors such as retention of skills.

2. It will permit testing the hypothesis that an inappropriate test was used in the current study. Tests suggested by the project director will be used in the follow-up study.

3. It will provide evaluative feedback in time to permit appropriate administrative action for the following year.

In view of the above plans, it would seem premature to draw conclusions or make recommendations prior to the completion of the projected follow-up study.

From a subjective standpoint, Research personnel associated with this project felt that it had a better chance for success than the project it supplanted. Goals and objectives were more narrowly defined, policies and procedures were more uniform, and the overall coordination and supervision of the project seemed to be more effective. The results of this evaluation are disappointing inasmuch as there seemed to be good reason to expect the project to be more successful than is indicated by the present evidence. It is to be hoped that the follow-up study will provide evidence which will point in a more positive direction. It should be pointed out, however, that the present study had sufficient merit to warrant regarding its findings with serious concern until such time as they are confirmed or refuted by the follow-up evaluation.

APPENDIX

APPENDIX

Statistical Procedures

A two-way (treatment by school) Manova run was made using pretest IQ and reading achievement as covariates. The dependent measures were posttest scores on Reading Vocabulary and Comprehension. The tests for the equality of the regression coefficients were statistically significant. The data were rerun separately by schools in another regression analysis program and it was found that the regression planes for treatment groups could be regarded as parallel within schools. Finally, the data were rerun in the Manova program with the same factors and variables. This time, however, a "pseudo" nested model was used (treatments nested within school). This resulted in lumping the variance due to treatment and interaction into a "pseudo" nested effect. Variance solely due to the school factor was partitioned as before. Tests of the significance of the differences between treatment groups within each school were provided by application of these procedures. In other words, the nested effect was partitioned by treatment groups within schools. Since a common regression plane could not really be fitted for all schools, separate tests of differences between treatment means within each school were also made as part of the regression procedures. Both the regression analyses and the Manova runs produced similar results. The contrasts reported in Tables 3 and 4 were taken from the Manova output. Had nonhomogeneity of regression mattered greatly, the constants based upon the regression planes unique to each school would have been reported instead of the Manova contrasts.

ORIGINAL ASSIGNMENT, ATTRITION, AND FINAL SAMPLE
FOR THE TITLE I PROJECT (1970-71)

Sixth Grade

		School 1	School 2	School 3	School 4	School 5	School 6	School 7	School 8	School 9
Experimental	Originally Assigned	53	32	42	69	34	33	32	42	59
	Dropped or Withdrawn	22	10	16	28	6	12	8	11	17
	Final Sample	31	22	26	41	28	21	23	31	42
Control	Originally Assigned	30	30	22	61	17	27	30	24	60
	Dropped or Withdrawn	13	12	15	15	5	8	10	7	17
	Final Sample	17	18	11	43	12	19	19	16	43

Seventh Grade

		School 1	School 2	School 3	School 4	School 5	School 6	School 7	School 8	School 9	School 10	School 11
Experimental	Originally Assigned	47	25	62	50	87	30	34	61	61	59	50
	Dropped or Withdrawn	19	3	24	9	21	15	7	13	10	14	14
	Final Sample	29	22	39	41	66	15	27	48	51	45	46
Control	Originally Assigned	30	23	32	47	54	39	30	55	56	45	26
	Dropped or Withdrawn	11	14	16	12	20	16	8	15	22	22	10
	Final Sample	19	9	16	35	34	23	22	40	34	23	16